The Cryosphere Discuss., 9, C511–C513, 2015
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The Cryosphere
Discussions
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Interactive comment on "Exploring the utility of quantitative network design in evaluating Arctic sea-ice thickness sampling strategies" *by* T. Kaminski et al.

Anonymous Referee #1

Received and published: 21 April 2015

General Comments

This paper presents a demonstration of a technique (quantitative network design) used to estimate the sensitivity of an ice-ocean model to the assimilation of particular observations. Here, the sample observations used are two flight transects from NASA's IceBridge airborne ice surveys. The study is well motivated and provides a useful demonstration of the potential benefit of this type of approach to better understand the sensitivity of ice-ocean forecasting systems to particular observations. While the specific conclusions drawn here with respect to Icebridge observations are not surprising or particularly new, the fact that they were derived objectively highlights the important potential of this type of approach.

The manuscript nicely justifies their approach and methods used, including the parameter choices and sensitivity thereto. My only concern is with respect to the degree to which the results are sensitive to model resolution. The authors even suggest that the “response functions” of their model could be used with other models to assess the observational impact on an assimilation system from a different model. I have difficulty understanding how the response functions would not be sensitive to the representation of different processes and thus resolution. This is of particular concern given that this study uses a relatively coarse resolution (vertical and horizontal) and thus may not be representative of the current generation of operational ice forecasting models. Additionally, how does resolution affect the usefulness of the QND results? Are they sensitive to the particular model state ? Can these methods be applied to an eddy-resolving model ? Current ice-ocean forecasting systems for the Arctic are approaching kilometer-scale, does this pose computational constraints for this method?

If the authors could adequately address this concern, and the minor specific points below, I think the article would be acceptable for publication.

Specific Comments

1. Section 2.2: While this section is very clearly written, I think it would benefit from further clarification and detail. Given that this paper aims to demonstrate the usefulness of a new method, providing more detail on the assimilation (and QND) description would help the reader follow the details and apply the method.

2. Section 2.3: Again, while I understand the method has been described in Kaminski and Rayner (2008) it would be helpful to elaborate, and possibly provide further explanation and/or examples to guide the reader.

3. Section 3.2, line 25: By determining the SD for the model in this way it excludes model bias, which for a relatively coarse resolution model of the Arctic is highly unlikely to be the case. Moreover, it likely underestimates the model error and neglects bias and resolution contributions. Why not use model-observation differences over the 20yr

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period, or differences between this model and a higher resolution reanalysis? Similarly, the error in surface boundary conditions is also likely underestimated.

4. Pg 1749, line 6: As noted above, I have difficulty understanding how the representation of nonlinear processes (such as eddies) would not impact the response functions. The issue of resolution needs to be addressed.

Technical Comments

1. Section 2.2, line 25: “is the transposed” should read “is the transpose”

2. Section 3.2, line 23: “SD” not defined

3. Section 4, pg 1745, line 7: “C2F surpasses B2F . . . ”. I agree with this statement but this pertains to 91d. It would be better to specify this directly as in the following sentence.

4. Page 1747, line 13: Again, it would be good to state clearly this is for the NOB target region, e.g. “. . .with respect to various impact factors for the NOB target region.”

5. Figures 5, 8, 10, and 11 are quite small and difficult to read. The article would benefit from some further improvement of the readability of these figures.

Interactive comment on The Cryosphere Discuss., 9, 1735, 2015.